

Robinson

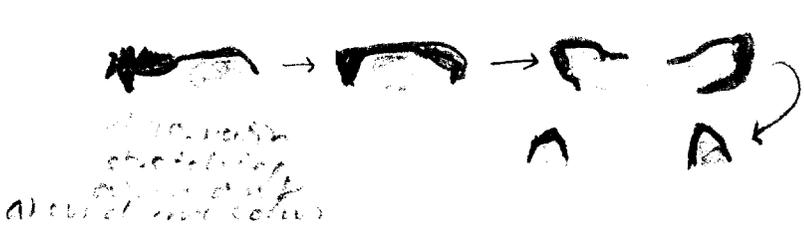
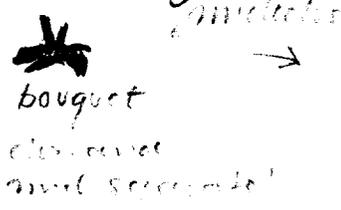
New York City

Jan. 12, 1958

Dear Joshua,

Many thanks for your welcome letter which caught me with one foot already on the train to N.Y. I am pleased to hear that you find mitosis in fungi worth investigating. Mitosis in growing hyphae of Neurospora crassa has been studied by my former student Dr. A. Bakerspigel who has a paper about it coming out soon in the A.J. Botany. The story is peculiar but its oddity is of the same order as that of the strange Englena "mitosis" which Ledale described last year in Nature and probably not sufficiently exotic to rouse the interest of geneticists in general. As I remember it the course of events runs something like this:

Robinson, C.



The bouquet stage is much more chromosome-like than anything one ever sees in the Mucorales but it is still not possible to say precisely what it means. Experiences with Allomyces, of which I have a stack of Feulgen pictures with me, suggest to me that in the bouquet phase homologous chromosomes are paired. Presumably they divide afterwards but if they do B's preparations are not yet transparent enough to show this.

Allomyces has excellent countable chromosomes but again there is a very peculiar form of division. More like that in Engelmann than like mitosis in the bean root. And of course none of it is visible in conventional iron hematoxylin slides.

In Schizophyllum, a basidiomycete one would think of as more sophisticated than Neurospora, B. has found that the divisions of the nuclei, which are comfortably large, are brutally direct.

Thus:



at rest

at rest

at rest

at rest

One more point that might interest you is that the nuclei in the phialides (sterigmata) of Penicillium, which are quite reasonably large, do not even possess a nucleolus. They look like so many balls of chromatin wool and they too divide by simple constriction. One sister nucleus going into the youngest conidium. — I find these observations fascinating because of the possible relationship between fungi and protozoa which they suggest but I fear this information is not important to geneticists.

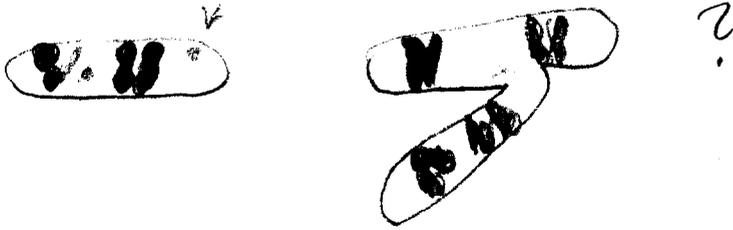
The puzzle of the behaviour of the chromatin bodies during fertilisation is constantly before me. I find nothing wrong with the photographs(?) which you had in the J. Bact. except that it does not suggest to me that the nuclei participate in fertilisation. With suitable Hfr strains it ought surely be possible to obtain enough material for looking into this. It might be worth while to cause a contraction of the nuclei first with chloromycetin. If fertilisation still occurs under these conditions participation of the chromatin bodies would be more or less ruled out.

Case 12:
Sincerely yours
C. M. R. M.

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With best wishes to both of you
for the progress of your work
and your fervent hope that you

I think it more likely, and see faint
evidence of it in your photographs,
that the small "centrioles" slip across
and I hope you will keep a look out
for this.



The "mitochondrial equivalents" of W. Nicklowitz,
Zbl. Bakt. I. Orig. 173, 12 (1958) might
in reality be the same thing as the centrioles
of H. G. Gierusa and Fentgen slides. Their
position fits and this idea would also
explain why union is end to side.
In other words, is there something like
a micronucleus??

While we are about it, can you tell me what
happens to the chromatin of the donor
cell after copulation? I realize that they
may be hard to identify but perhaps
the recipients could be lysed away
by phase at the right moment, thus
tagging the donors.

I should of course very much like to
talk these matters over with you by the side
of a laboratory bench. Perhaps we shall
penetrate sufficiently far West next summer

to make it possible. I have a talk to give in Ann Arbor in July.